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Huber, Lars C ; Müller, Véronique

DOI: <https://doi.org/10.1056/NEJMc1009061>

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ZORA URL: <https://doi.org/10.5167/uzh-46122>

Journal Article

Originally published at:

Huber, Lars C; Müller, Véronique (2010). Acute pulmonary embolism. New England Journal of Medicine, 363(20):1972-author reply 1974.

DOI: <https://doi.org/10.1056/NEJMc1009061>

Acute Pulmonary Embolism

TO THE EDITOR: In their review article, Agnelli and Becattini (July 15 issue)¹ discuss many important topics in acute pulmonary embolism. We would like to highlight the importance of the use of pulmonary magnetic resonance imaging (MRI) in the diagnosis of this condition, since substantial technical developments have been introduced in recent years.^{2,3} Continued improvements include the use of parallel imaging, angiography technique, and pulmonary perfusion,²⁻⁴ with the latter showing the most promise for the diagnosis of pulmonary embolism.³ However, even in protocols without pulmonary perfusion, large studies have shown good results with the use of MRI.⁵ Overall, the combined MRI protocol was both reliable and sensitive, with results similar to those obtained with 16-slice multidetector computed tomography (CT).³ MRI has potential advantages over CT, including a radiation-free method, a better safety profile for contrast material, and capability of functional imaging.⁴ MRI might therefore be considered as a valuable alternative in the assessment of suspected pulmonary embolism in certain groups of patients.

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No potential conflict of interest relevant to this letter was reported.

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TO THE EDITOR: Agnelli and Becattini suggest that ventilation-perfusion lung scanning should be limited to situations in which multidetector CT is either unavailable or contraindicated. However, nuclear medicine technology has evolved since the era of the original Prospective Investigation of Pulmonary Embolism Diagnosis (PIOPED) study, with advances such as multidetector scanners, three-dimensional imaging (including single-photon-emission CT [SPECT]), improved software, hybrid SPECT-CT devices, and better ventilation agents (with the latter now available in many countries although, notably, not in the United States). The authors cite data relevant to planar ventilation-perfusion imaging, a technique long superseded by SPECT in many centers in Europe, Canada, and Australia because of its increased sensitivity, specificity, and reproducibility and its low nondiagnostic rate.¹ Studies have consistently suggested a higher sensitivity for ventilation-perfusion SPECT (>95%) than multidetector CT (68 to 86%),^{2,3} although prospective multicenter trials have yet to be conducted.⁴ Ventilation-perfusion scintigraphy with the use of modern techniques, such as SPECT, has several advantages over multidetector CT,² including a higher sensitivity, a lower dose of radiation to the breast, a lower risk associated with contrast material, and a greater number of technically adequate studies. Thus, the use of this technology should be part of the imaging algorithm for the diagnosis of pulmonary embolism today.

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TO THE EDITOR: We have a different opinion regarding the need for ventilation-perfusion scanning or CT in patients with a high clinical suspicion of pulmonary embolism who are undergoing positive duplex ultrasonography of the lower limbs. The authors conclude that no further lung imaging for definitive diagnosis of suspected pulmonary embolism is necessary in such patients and that practitioners should proceed directly to anticoagulation. The difficulty arises when this same patient presents at a later date with a possibly new embolus. One must establish whether this episode represents a recurrent lung embolism or a nonembolic respiratory problem. We agree with the suggestion of Fedullo and Tapson¹ that lung imaging at the time of the first presentation would answer this question. The use of such imaging would obviate the need for lifelong anticoagulation in case a recurrent embolus is ruled out.

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1. Fedullo PF, Tapson VF. The evaluation of suspected pulmonary embolism. *N Engl J Med* 2003;349:1247-56.

TO THE EDITOR: Agnelli and Becattini recommend a strategy of clinical-probability assessment, D-dimer testing, and (if necessary) multidetector CT for patients whose condition is hemodynamically stable and who have a low clinical probability of pulmonary embolism. For patients in this category who have none of the Pulmonary Embolism Rule-out Criteria (PERC), further diagnostic testing may be avoided.¹ The PERC rule (age, <50 years; pulse, <100 bpm; oxygen saturation, ≥95%; and no unilateral leg swelling, hemoptysis, recent surgery, hormone use, or

previous venous thromboembolism) has been prospectively validated with a sensitivity of 97.4% in patients at very low risk.² The D-dimer assay may be falsely positive in a variety of conditions other than pulmonary embolism,³ leading to unnecessary CT imaging. Given the importance of reducing morbidity, mortality, and financial costs from unnecessary radiation exposure, as illustrated by the 2010 Food and Drug Administration initiative,⁴ the PERC rule warrants consideration in the evaluation of pulmonary embolism in patients with hemodynamic stability and a low clinical probability of pulmonary embolism.

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No potential conflict of interest relevant to this letter was reported.

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TO THE EDITOR: We are surprised that Agnelli and Becattini do not mention the role of electrocardiography in the diagnosis of pulmonary embolism. Even in the era of sophisticated procedures, electrocardiography probably remains the first technical investigation undertaken in patients with symptoms of pulmonary embolism, in particular when presenting with chest pain. Distinct electrocardiographic signs, although limited in the diagnostic workup, have clearly been associated with right ventricular strain and dysfunction. Anterior T-wave inversion was found to be associated with increased mean pulmonary arterial pressure,¹ and the development of QR in V1 was identified as an independent risk factor for an adverse prognosis.² In clinical practice, the presence of these signs may push toward an aggressive approach and shorten the time to intravascular thrombolysis. Electrocardiography still

has value in the management of acute pulmonary embolism, in particular in risk stratification.

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TO THE EDITOR: Agnelli and Becattini state that “surgical embolectomy should be restricted to high-risk patients with an absolute contraindication to thrombolytic treatment and those in whom thrombolytic treatment has not improved hemodynamic status.” We strongly disagree. Studies have suggested that surgical pulmonary embolectomy should also be considered early in the management of acute, massive pulmonary embolus in hemodynamically stable patients with right ventricular dysfunction who have large central clots involving the main pulmonary artery and its branches. Operative mortality in such patients is 6 to 8%.^{1,2} In contrast, patients who have sustained a cardiac arrest, who require cardiopulmonary resuscitation or high-dose inotropes, or who have multisystem failure have rates of death of more than 50%.³ Retrospective studies have shown that thrombolytic therapy results in a higher rate of death, an increased risk of major hemorrhage, and a higher recurrence rate of pulmonary embolism than does surgical embolectomy.⁴ Surgical embolectomy is an effective therapeutic option for the treatment of acute, massive pulmonary embolism, and under the proper circumstances, it should be considered first-line therapy, not the last resort.

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No potential conflict of interest relevant to this letter was reported.

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THE AUTHORS REPLY: Hochhegger et al. assert that recent technical developments in pulmonary MRI qualify this procedure for an upgrade in the diagnostic algorithms of pulmonary embolism. This change would be particularly welcome in light of potential safety advantages of MRI over multidetector CT. However, whether these improvements would result in a clinical advantage needs to be assessed in large-scale, prospective studies.¹ These considerations also apply to ventilation–perfusion SPECT, which can improve the performance of the traditional planar ventilation–perfusion imaging,² as noted by Roach and Bajc. Both MRI and ventilation–perfusion SPECT share the challenge of requiring around-the-clock availability in hospitals.

Whether patients with proven proximal deep-vein thrombosis should proceed to further testing to confirm suspected pulmonary embolism, as suggested by Sfedu et al., is a common clinical discussion. Indeed, although lung imaging may facilitate risk stratification and patient follow-up, it will not change treatment in a large majority of patients.³

Rosenson supports the PERC rule to avoid D-dimer testing and CT to exclude pulmonary embolism in hemodynamically stable patients with low clinical probability. This approach might be considered in patients with definitively low clinical probability, keeping in mind that it has not been validated in management studies.

Huber and Müller question the limited role that we assigned to electrocardiography in the treatment of patients with acute pulmonary embolism. They are correct in that electrocardiography is crucial in patients presenting with chest pain or dyspnea and could be incorporated into the assessment of the clinical probability of pulmonary embolism. However, electrocardiography needs to be followed by additional testing for diagnosis, as well for risk stratification for an

adverse outcome. Indeed, to our knowledge, no intervention study has been performed to evaluate the clinical benefit of more aggressive management of acute pulmonary embolism on the basis of electrocardiographic findings.

Lazar and Farber propose to extend surgical embolectomy to hemodynamically stable patients who are at moderately high risk for pulmonary embolism. They also express concern about the increased risk of bleeding associated with thrombolytic therapy. A large, randomized trial, the Pulmonary Embolism International Thrombolysis Study (ClinicalTrials.gov number, NCT00639743), is ongoing to explore the clinical benefit of thrombolysis in such patients. A similar prospective study should be performed with respect to surgical embolectomy for acute pulmonary embolism before that approach can be considered as a first-line therapy rather than as a procedure

for selected patients in selected medical institutions by experienced surgical teams.

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Since publication of their article, the authors report no further potential conflict of interest.

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Accountability Measures to Promote Quality Improvement

TO THE EDITOR: Chassin et al. (Aug. 12 issue)¹ describe the recent history of accountability measures in hospitals, which, they argue, promote quality improvement. But measurement for the purpose of accountability or judgment and measurement for the purpose of improvement of health care processes are two very different things.² Properly understood, the two approaches can play complementary roles in advancing organizational goals. However, confusing measurement for accountability with measurement for improvement can only give rise to organizational confusion and frustration.

First, measurement does not, as implied in this article, equate with improvement. Improvement requires making changes to health care processes and structures. Second, measurement for improvement is not focused on judging whether data meet a compliance threshold or target. Instead it is directed toward determining whether the changes we make to improve something work and to what degree they work. Furthermore, quality improvement incorporates sets of related measures (process, outcome, and balancing) to help us understand the broader effect of the changes tested.

Without clearly differentiating measurement for accountability from measurement for im-

provement, health care professionals could easily confuse the ends with the means.

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THE AUTHORS REPLY: We agree with Perla et al. that measures used for accountability, such as those used in public reporting, accreditation, or pay for performance, can differ from measures used for local improvement. But the two must be closely aligned if the measures used by accountability programs are to stimulate the kinds of process-improvement activities that lead directly